also have to balance the risks of the vaccine against the benefits of immunisation in the community, and this is the true role of epidemiology. The other problems are clinical or pathological. But in an attempt to adapt them to epidemiological investigation oversimplifications have been introduced which obscure the fundamental issues. The most important of these oversimplifications is the use of "convulsions within 72 hours of inoculation" as a convenient criterion of vaccine damage.

The confusion began with Miller and Stanton's description of the typical neurological reaction.[4] "Clinically," they wrote, "the picture is a striking one, most commonly of convulsions occurring 20 minutes to 72 hours after injection, followed by coma and hemiplegia or both." Attentive reading of their citations, however, will show that the cases on which they based this description were selected *because* they presented with convulsions. All non-striking presentations, immediate and delayed, were consequently overlooked. In the course of repeated summaries and transcriptions the rule then became "pertussis vaccine encephalopathy begins with convulsions"; then "Brain damage is not attributable to the vaccine unless there were convulsions within 72 hours of inoculation"; and finally the coma and hemiplegia were split off and discarded and the syndrome became indistinguishable from the benign febrile convulsions of infancy. It could then be statistically proved that whooping cough vaccine was not a cause of febrile convulsions, when nobody claimed that it was.

It is still possible that any of the reported neurological consequences of the parenteral use of any foreign protein or any vaccine, or of the toxins which are active in whooping cough itself, may in time prove to occur after whooping cough vaccine. It is also possible that in children liable to convulsions brain damage from the vaccine manifests itself in the form of convulsions, whereas in children not so liable the onset is less striking. Until such questions and a host of others are settled it is too early to think of balancing the risks.

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[1] Werne, J, and Garrow, I, *Journal of the American Medical Association*, 1946, **131**, 730.
[2] Byers, R K, and Moll, F C, *Pediatrics*, 1948, **1**, 437.
[3] Brody, M, *Journal of the American Medical Association*, 1949, **140**, 555.
[4] Miller, H G, and Stanton, J B, *Quarterly Journal of Medicine*, 1954, New Series **23**, 1.

SIR,—Dr J S Robertson (22 September, p 735) has hit the nail—or in this case the needle—on the head, in his criticisms of Professor Gordon Stewart's analysis of neurological ills from whooping cough vaccine.[1] Five years ago Prensky[2] with similar arguments showed the weakness of the case against pertussis immunisation. It is thus more than unfortunate that Stewart has continued to propagate his opinions without adding substantial new evidence.

I have had the opportunity to investigate the nature of pertussis immunisation convulsions,[3] using the technique previously used to differentiate "anoxic" from "epileptic" febrile convulsions.[4] Of 12 consecutive children with pertussis immunisation convulsions seen over a two-year period, 11 had excessive asystole on ocular compression. This exaggerated vagal response to ocular compression in pertussis immunisation convulsions was indistinguishable from that found in "anoxic" febrile convulsions, but significantly different from that in "encephalopathic" febrile convulsions ($P < 0{\cdot}01$, multiple linear regression analysis). Although pertussis-containing vaccines are undoubtedly reactogenic,[5] my study indicates that pertussis immunisation convulsions resemble cardiogenic "anoxic" seizures (fainting fits)[6] and are not evidence of any direct encephalopathic effect of the injected substances.

Since vagal anoxic seizures are preventable by atropine sulphate[6] or atropine methonitrate (unpublished observations), prophylaxis of pertussis immunisation convulsions may be a practicable proposition and deserves study.

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[1] Stewart, G T, *Journal of Epidemiology and Community Health*, 1979, **33**, 150.
[2] Prensky, A L, *Developmental Medicine and Child Neurology*, 1974, **16**, 539.
[3] Stephenson, J B P, *Lancet*, 1979, **2**, 416.
[4] Stephenson, J B P, *British Medical Journal*, 1978, **2**, 726.
[5] Barkin, R M, and Pichichero, M E, *Pediatrics*, 1979, **63**, 256.
[6] Stephenson, J B P, *Archives of Disease in Childhood*, 1978, **53**, 193.

Rubella vaccination

SIR,—Dr Nicholas Black (22 September, p 735) advocates the serious consideration of immunisation against rubella without serotesting. In support he quotes figures by Mayon-White and Bull,[1] who found only 50% acceptance for serotesting.

It would have been fairer had he also quoted papers showing a much higher acceptance rate. For instance, the paper by myself and my colleagues[2] reports a study in which virtually all the patients approached agreed to be tested and where only two patients out of 55 did not return for vaccination. This would have produced cost figures very different from the £20 a conversion quoted by Dr Black.

Dr Black states "that immunising without serotesting is not without medicolegal difficulties." I feel we should be worrying more about the ethical considerations of producing terminations where we need not have done so had we ignored the cost.

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[1] Mayon-White, R T, and Bull, M J V, *Practitioner*, 1976, **216**, 317.
[2] Gringras, M, *et al*, *British Medical Journal*, 1977, **2**, 245.

Circumcision and cruelty to children

SIR,—Conscientious surgeons and paediatricians have listed the complications of neonatal circumcision both in this journal (Drs P M Fleiss and J Douglass (1 September, p 554)) and in other scientific papers. Without anaesthetic the operation in babies causes pain, intense and prolonged crying, air swallowing, vomiting sometimes followed by apnoea, and sometimes permanent local complications. With anaesthetic "the procedure should *not* be considered minor," because of both local and anaesthetic complications. There is a small risk of death following circumcision, which means that there must be clear-cut immediate medical indications to justify that risk. Some arguments in favour of circumcision for long-term reasons smack of sophistry.

Drs Fleiss and Douglass dispassionately described a cruel practice. If circumcision without anaesthetic had not been sanctioned by authority, there would be grounds for criminal prosecution of the operator for inflicting unnecessary cruelty, and grounds (in the UK) for a care order removing the child from parental authority. Indeed, such action has been taken in the UK against parents who inflict bizarre (but less common) tribal mutilations on their young children. Doctors do not supervise or administer facial cicatrisation (with the appropriate sterile precautions and antibiotics) to help the child identify with his ancestral tribe. They do not remove toes because of a long-term risk of bunions, poor hygiene, ingrowing toenails, or toe cancer.

If circumcision for non-medical reasons has died out in Scandinavia and is dying out in the UK, why has the practice persisted in parts of the UK and much of the United States? Do religious and social pressures perpetuate unnecessary circumcision (or any other unnecessary mutilation or operation)? It may be true that the stress to a child in a Moslem enclave in the UK would be great if he were excluded from being the same as his peers, and made a pariah from his group. If this is correct, cannot circumcision for religious (or social) reasons be delayed until 16 years, when the young man can take the positive decision? He may then be determined to accept risk and pain for the sake of his beliefs. It just seems wrong for authority to continue to encourage certain parents to subject *young* children to unnecessary risk, pain, and distress, and for *young* children to be denied any protection from the law just because a malpractice is rather common.

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Risks of amniocentesis

SIR,—The review article on amniocentesis by Barbara J Culliton and Wallace K Waterfall (22 September, p 723) is based on part of the report of the Consensus Development Conference on Antenatal Diagnosis[1] which took place at the US National Institutes of Health in March 1979. The article, like the report, refers briefly to the Medical Research Council's assessment of the risks of amniocentesis,[3] both references carrying the implication that the MRC findings can be ignored because of methodological shortcomings, and thus do not cast doubt on the general conclusion that "midtrimester amniocentesis is both safe and effective," as they say in their article.

We wish to make two points. Firstly, this was an internal conference with no representation from any country outside the United States, so that the "consensus" does not necessarily extend to other countries. Secondly, a number of methodological criticisms of the MRC study from the United States have been raised since the conference,[3 4] and have been shown to be without foundation.[5 6]

The MRC study of the risks of amniocentesis